Clim. Past Discuss., 8, C2220–C2223, 2012
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Climate of the Past Discussions

Interactive comment on "What could have caused pre-industrial biomass burning emissions to exceed current rates?" *by* G. R. van der Werf et al.

Anonymous Referee #3

Received and published: 6 November 2012

General comment:

The manuscript by van der Werf et al. aims at explaining why ice core CO reconstructions from the South Pole station suggest approximately 4 times the amount of CO emissions in the past relative to current levels of CO emissions. The authors point to savanna ecosystems of the Southern Hemisphere as a potential source for boosting CO emissions, emphasizing the importance of Fire Return Times (e.g. shorter return times produce more emission if sufficient time has elapsed for fuel build up). In order to generate sufficient CO emissions that match the findings in the South Pole ice core data, the authors present several extremely unlikely biomass burning scenarios (all savannas in the SH must burn annually or bi-annually plus extreme contributions from high rates of deforestation and slash & burn agriculture) to explain the ice core

CO record. In general, the manuscript is well written and organized around the three methodological approaches; (1) GFED 3 and the chemical transport model, (2) historical fire emissions since 1400 AD, and (3) simulations on savanna ecosystems to capture Fire Return Times and relative CO emissions. The paper is provocative in that it calls upon the ice core community to replicate the South Pole CO reconstructions of Wang et al. 2010 and sedimentary charcoal community to provide more detailed (spatial and temporal) coverage of fire reconstructions from the SH, and particularly savanna ecosystems. This manuscript is suitable for publication providing the authors are willing to address several minor suggestions listed below.

Specific comments:

Page 3161, line 23: "...especially in the tropics, humans start most fires." This assumes that lightning has a reduced role in ignition for tropical environments, but based on NASA OTD/LIS data, the highest global density of lightning occurrence can be found over tropical regions. Considering most tropical lightning arrives with wet convection, the argument for human versus lightning ignition in the tropics will require further evaluation. The statement that most fires are started by humans is an assumption that has not been confirmed.

Page 3161 line 26-27: Drivers of the 15^{th} century downturn in biomass burning was recently revisited by Power et al. 2012 (the Holocene) and provides arguments for climate-driven changes in biomass burning, and does not support the conclusion that demographic collapse in the America's explains the decrease in fire activity. Also, this study provides new interpretations using charcoal-based fire reconstructions from southern South America that could inform some of the results in this manuscript.

Page 3165, line 18: The" TM5 chemical transport model was driven by ECMWF meteorological fields..." the acronym ECMWF is not explained in the text, though I assume it refers to the European Centre for Medium-Range Forecasts.

Page 3165, line 26: By using the average CO mole fraction at the South Pole station

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from the years 2002-2007, a period of large inter-annual variations (e.g. breakup of Larsen B ice shelf in 2002) assumes that average conditions contribute disproportionately to most fire emissions, whereas research in Amazonia suggest that anomalous conditions (drought linked to strong El Nino Southern Oscillation episodes) contribute disproportionately to total area burned and subsequent emissions (e.g. Nepstad et al. 2004 Global Change Biology). Recognizing that inter-annual variability contributed to less than 3% (shown in Table 1) in the sensitivity analysis, changes in large-scale circulation were likely significant factors in long-term emissions (but difficult to quantify as mentioned on Page 3179:line 1)

Page 3166, lines 4-5: It is unclear why the authors assumed there is no seasonal cycle in fuelwood emissions. In higher latitudes, one would expect a stronger seasonal signal from fuelwood combustion during winter months. A brief clarification would be helpful for why the seasonal fuelwood was assumed constant.

Pages 3167-3168: The model developed to identify savanna emissions, driven in part by Mean Annual Precipitation (Huffman et al. 2009) and MAT (Hansen et al. 1999), suggests that the wet season increases grasses, which then become susceptible to burning in the dry season. The use of MAP (versus seasonal or monthly precipitation) may obscure the fact that some savanna regions can have two wet seasons and two dry seasons, resulting in greater variability in savanna fuel production and fire return times in response to inter-annual changes in moisture. How would an alternative approach (using seasonal moisture availability versus MAP) influence NPP and emission scenarios for savanna systems?

Page 3169, line 23: Typo, remove the second use of the word “ratio” (high SPO mixing ratios ratio to emissons. . .)

Page 3174, lines 17-23: Research on fire in recently cleared tropical forests suggests that fire frequency and total area burned tends to increase significantly after the first time a forest patch has been cleared and burned by “fire creep”(e.g. Cochrane and

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Barber 2009 Global Change Biology). Emissions from slash and burn agriculture may accelerate over time based on the interpretations of Cochrane et al. and may contribute a larger proportion of CO than assumed by scaling the CO emissions with rural population data. A sentence or two inserted could address this uncertainty around the time-transgressive nature of fire emissions once a tropical forest has been initially cleared and burned.

Page 3174, line 28: "there is clear evidence of elevated use of fire in earlier time periods" This statement should be qualified with a specific example or citation, because not all researchers working in Amazonia would agree that there is clear evidence of elevated use of fire. For example, a recent *Science* paper by C. McMichael et al. (2012) has received much attention (+ and -) because of the controversy around this issue.

Page 3175, line 1: The body of evidence that suggests a long history of human-induced disturbance in soils and lakes from the Amazon is relatively small (I would delete "substantial" from the sentence) and the sedimentary charcoal records from soils are more problematic in terms of chronologies and interpretation than those from lake sediments (hence the controversy around the McMichael et al. 2012 paper in Science).

Page 3176, line 20-23: A synthesis of all evidence of fire activity in Australia has recently been published by Mooney et al (2011 Quaternary Science Reviews), suggesting that the role of humans in biomass burning in Australia is minimal, at least until the last 200 years.

Table/figure comments:

Tables- fine

Figure 1 (page 3193): Typo in the figure legend, remove the word "data". "The CO data ice core data. . . "

Interactive comment on Clim. Past Discuss., 8, 3159, 2012.